Review: Space-Time Physics and the Philosophy of Science
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SPACE-TIME PHYSICS AND THE PHILOSOPHY OF SCIENCE*

In a lecture delivered at Göttingen in 1907, the mathematician Hermann Minkowski showed that the Principle of Relativity and the Principle of the Constancy of Light Velocity postulated by Einstein in 1905 followed from the assumption that all physical events are located in a four-dimensional Riemannian manifold, with a flat metric of signature $(+---)$, in which light signals *in vacuo* describe null curves ($ds = 0$), and inertial particles describe timelike curves ($ds^2 > 0$). Such a manifold is naturally decomposed by the choice of an inertial particle into a one-dimensional time manifold represented by the particle's timelike worldline and a three-dimensional space manifold represented by any hyperplane orthogonal to the said worldline. In this fashion, the first explicit theory of 'space-time' came into being.

Einstein, who initially looked askance on the geometrical interpretation of his ideas by his former teacher Minkowski, in the end embraced it wholeheartedly, when he realised that it paved the way for a proper handling of the phenomena of gravitation, which were a major stumbling block for his Relativity theory. Indeed, the theory of gravity published by Einstein on 25 November 1915 deserves its given name of General Relativity (*GR*) only insofar as it generalises the flat Riemannian manifold of his earlier theory—now dubbed Special Relativity (*SR*)—to a Riemannian manifold of variable curvature dependent on the distribution of matter. ('Relativity', in a more familiar acceptance, is tantamount to equivalence of frames. Thus, Einstein's Principle of Relativity of 1905 implies that any pair of physical experiments carried out under homologous conditions in two labs moving inertially relatively to one another lead to homologous results. But, as Michael Friedman makes abundantly clear in his book, *GR* does not proclaim a *general* equivalence of frames, for it certainly does not entail the preposterous conclusion that one can never tell, by means of physical experiments, one arbitrarily chosen lab from another.)

In 1923 another great mathematician, the Frenchman Elie Cartan, using the latest advances in differential geometry, due to Levi-Cività [1917], Hessenberg [1917], Weyl [1918] and himself, reformulated Newton's Gravitation Theory (*NG*) as a space-time theory in which the 'arena' of physics is made to consist not indeed in a Riemannian manifold (*i.e.* one in which all curves have 'lengths'), but in a four-dimensional manifold endowed with a so-called linear connection (which selects some curves as the analogue of Euclidian straights, *i.e.* as lines that keep a steady 'direction'),

* Review of Michael Friedman [1983]: *Foundations of Space-Time Theories, Relativistic Physics and the Philosophy of Science*. Princeton University Press. xvi + 385 pp.
This article was written while I was a visiting fellow at the Center for Philosophy of Science of the University of Pittsburgh. I am grateful to the Center for its hospitality and its stimulating atmosphere. I also wish to thank David Malament and John Norton for their very helpful criticism of my manuscript.

dependent—like *GR*'s variable curvature—on the distribution of matter. The new advances in geometry had shown that the difference between a flat and a non-flat manifold depends altogether on the linear connection, and follows from a given Riemannian metric only in so far as the latter uniquely determines a (particular type of) connection. Since gravitational phenomena in both *GR* and *NG* are a manifestation of space-time curvature, Cartan had found in effect a common conceptual denominator for these two theories, thus exposing the myth of their incommensurability forty years before it saw the light. Finally, as has been shown, among others, by Friedman himself, in a paper written jointly with John Earman [1973], one can readily formulate Newtonian Kinematics (*NK*) as a space-time theory by suitably simplifying the postulates of Cartan's *NG*. (This may sound like hunting doves with heavy ordnance, but it is philosophically interesting, and also, as Friedman shows in his book, didactically quite useful.)

Due to Reichenbach's neglect of modern geometry—pointedly noted by Weyl [1924] in his review of the former's *Axiomatik der relativistischen Raum-Zeit-Lehre*—philosophers who have picked up their understanding of Einsteinian Relativity from Reichenbach's *Philosophy of Space-Time Theory*[1]—instead of, say, from Weyl's *Space-Time-Matter*—have failed to grasp the right conceptual links between *NK*, *NG*, *SR* and *GR*, and have therefore tended to see the chasm between Einstein's thought and Newton's as much wider and deeper than it is. Reichenbach's views on space and time have been strongly criticized in recent philosophical books (Nerlich [1976], Angel [1980]), and the meagreness of their scientific foundations should be obvious to any student of a good modern standard textbook of Relativity (*e.g.* Misner, Thorne and Wheeler's *Gravitation*, Track One). But there was still a need for a work like Michael Friedman's, which provides a fairly complete and rigorous presentation of the conceptual framework of the space-time theories of mathematical physics, followed by a clear, detailed, convincing, and in some important respects original discussion of their chief implications for natural philosophy and for the very idea of a physical theory. (The latter will, one hopes, finally dispel the illusion that Relativity physics lent effective support to the philosophy of logical empiricism.) The book is designed for 'scientifically inclined philosophers and philosophically inclined scientists' (p.xi) with no more than a smattering of linear algebra and the calculus (the necessary differential geometry is explained in a very readable appendix—pp. 340—67), and should give them a sounder and more enlightened guidance to the Theory of Relativity and its philosophy than was hitherto available in works of this kind.

An introductory chapter (pp. 3—31) sets forth the general outlook and the aims of the book. These go farther than my above description may suggest, for Friedman also intends to study the interaction between the positivist and conventionalist tendencies of twentieth-century philosophy of science and

[1] Reichenbach [1928], translated into English as *The Philosophy of Space and Time*.

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the development of relativity, and to analyse some conceptual entanglements—*e.g.* between the Principle of Equivalence and the idea of general relativity—which he believes are noticeable in Einstein's work. While Friedman's writing on these historical questions (especially on the first one) is brilliant and certainly worth reading, the scholarly apparatus that he brings to bear on them is, I dare say, not sufficiently detailed to certify all his claims (at least with regard to Einstein's thought from 1905 to 1915).

The next four chapters (pp. 32—215) present the family of space-time theories and its best known members, namely, *NK*, *NG*, *SR* and *GR*. The chief common feature of space-time theories is that they conceive of physical processes and events as filling a region, or, in the limit, a hypersurface, surface, curve or point of a four-dimensional real differentiable manifold, which, following Minkowski, I shall call the theory's underlying *world*. (The prefix *world-*, as in *worldpoint*, *worldline*, *worldvelocity*, indicates appurtenance to this manifold. A space-time theory can have models built on several topologically inequivalent worlds.) Without going too far into mathematical minutiae, we can take note of some important properties of real n-dimensional manifolds:

(1) Any such manifold M is a topological space (*i.e.* a point set in which it makes sense to speak of neighbourhoods and continuity), each point of which has a neighbourhood that is topologically equivalent to an open ball in the n-dimensional Euclidian space $\mathbf{R}^n$. This means that a neighbourhood of each point P of M can be put into one-to-one bicontinuous correspondence with any open part of $\mathbf{R}^n$. Such a mapping of a neighbourhood of P in M into $\mathbf{R}^n$ is called a *chart* of M about P.

(2) The differentiable structure of M is given by a choice of such charts, subject to the twofold condition that (*a*) each point of M lies in the domain of at least one of them, and (*b*) the chosen charts are mutually compatible, so that the composition of a chart's inverse by any chart is, wherever it is defined, a smooth mapping of a part of $\mathbf{R}^n$ into $\mathbf{R}^n$. This enables one to define the concept of a smooth mapping of any differentiable manifold into another.

(3) Let P be a point of the manifold M, and let $F(P)$ denote the set of smooth real-valued functions defined on some neighbourhood of P, with the obvious algebraic structure given by the operations of addition of functions and multiplication of a function by a real number. Then, the set $F^*(P)$ of real-valued linear functions on $F(P)$ can be naturally structured as an infinite-dimensional real vector space. There are several essentially equivalent ways of selecting an n-dimensional subspace of $F^*(P)$, known as the tangent space at P, which I shall denote by T_PM. T_PM is a topological vector space isomorphic with $\mathbf{R}^n$, and can therefore readily be made into a differentiable manifold of the same dimension number as M.

The power of space-time theories rests almost entirely on the fact

mentioned under (3). It follows from it that each point P of a manifold M is associated not only with the tangent space $T_P M$, but also with its dual, $T^*_P M$, *i.e.* the space of real-valued linear functions on $T_P M$, and with all the infinitely many vector spaces of multilinear functions with arguments drawn from $T_P M$ and $T^*_P M$, and values in some specially designated vector space. The elements of these spaces are generally known as *tensors* at P, with some qualification indicative of the nature of their respective arguments and values. The collection of all tensors of a given type at every point of M is readily made into a differentiable manifold, the tensor bundle (of the said type) over M. A smooth mapping from M to this bundle, which assigns to each point of M a tensor at that point, is called a tensor field (of the type in question) on M. Another interesting differentiable manifold naturally associated with an n-dimensional manifold M is the so-called principal bundle of n-ads over M—an n-ad at a point P or M being an ordered basis of the tangent space $T_P M$. Since bundles over M are manifolds they also carry tensors at each point, have bundles over them and can have fields defined on them.

The space-time physicist thus has at his disposal a vast and varied array of mathematical objects from which to choose the intellectual representatives of physical realities. He can systematically compare the values of a chosen object at nearby space-time points by resorting to several differential operators, such as the Lie derivative and Cartan's exterior derivative, which are defined on arbitrary manifolds, or such as the Fermi derivative, Ricci's covariant derivative and covariant differential and Cartan's exterior covariant derivative, which are defined on manifolds endowed with some additional structure. Thus, for example, when classical electrodynamics is formulated as a space-time theory the electromagnetic field is conceived as a skew-symmetric (0, 2)-tensor field F;[1] half of Maxwell's equations are then packed into the simple formula $dF = 0$, where d stands for the exterior derivative. (The formula for the other half is no less concise, but requires further explanations.)

Tensor fields and their kin are often called 'geometric objects', but, at least from a historical point of view, some of them are more geometric than others, in so far as they can serve to specify in the underlying manifold the sort of properties and relations that have been traditionally studied by geometry. Thus, a (0, 2)-tensor field which assigns to each point of a manifold M a positive definite symmetric bilinear function on the respective tangent space (thereby making the latter into an inner product vector space) is all that is needed to define the length of curves, the size of angles, the

[1] A V-valued (q, r)-tensor at P is a $(q+r)$-linear function from the Cartesian product of $T^*_P M$ (taken q times) and $T_P M$ (taken r times) to the vector space V. If $V = \mathbf{R}$ we simply speak of a (q, r)-tensor. Thus, a (0, 2)-tensor t at P is simply a real-valued bilinear function on $T_P M \times T_P M$. It is symmetric if, for any pair of arguments x and y, $t(x, y) = t(y, x)$; skew-symmetric, if $t(x, y) = -t(y, x)$. A (0, 2)-tensor field is skew-symmetric (resp. symmetric) if all its values are skew-symmetric (resp. symmetric).

volume of regions and the congruence of figures in M. (Such a tensor field is called a proper Riemannian metric on M. If we relax the requirement of positive definiteness we obtain the kind of improper Riemannian metric that characterises the 'geometry' of SR and GR space-times.) Likewise, the choice of a certain kind of vector-valued (0, 2)-tensor field on the principal bundle of n-ads over M, known as a connection form for M, provides the ground for distinguishing between the ordinary curves of M and its so-called geodesics or curves of constant direction (*straights* in Euclidian and Lobachevsky space), and also furnishes the additional structure required for defining the Fermi, covariant and exterior covariant derivatives on M.

The space-time theories mentioned above all assume that their respective worlds are structured by geometric objects in the aforesaid narrow sense. NK, NG and SR neatly separate them from the geometric objects in the wider sense selected to represent physical realities, which are linked to each other—but not to the former—by the respective theory's field equations. The great novelty of GR is that it puts all its designated geometric objects on a par—the Einstein field equations of gravity tie the (improper Riemannian) space-time metric to the stress-energy tensor field representing the distribution of matter and non-gravitational energy. J. L. Anderson [1967] expressed this difference by saying that, while the earlier space-time theories postulate both *absolute* and *dynamic* geometric objects, in GR all postulated objects are dynamic. Anderson uses his notion of absolute objects to give a characterisation of the symmetry group of a space-time theory, as follows: The symmetry group of a theory T is the largest group of smooth permutations with smooth inverses of the underlying world(s) of T which leave the absolute geometric objects of T invariant.[1] Anderson is thereby enabled to offer a vindication of sorts of the name 'General Relativity', based on the following vacuous truth: GR's symmetry group is the largest—or 'most general'—conceivable, inasmuch as no permutation of GR's underlying world(s) can fail to leave invariant every absolute object of the theory, for there are no such objects at all.

Friedman approves of Anderson's general idea and repeatedly resorts to it when comparing the several space-time theories among themselves; but he offers a characterisation of absolute objects which is more precise than the rather informal one given above and is less obscure than Anderson's. It goes as follows. We conceive a space-time theory as a class of structures—the

[1] By a permutation of a set S I mean a one-to-one mapping of S onto itself. To understand how a smooth permutation of a differentiable manifold M can be said to leave invariant a given geometric object g on M, we must bear in mind that any such permutation f induces a linear isomorphism of the tangent and cotangent spaces at each point P in M onto, respectively, the tangent and cotangent spaces at $f(P)$. Let g be, for example, a (0, 2)-tensor field on M, assigning to each point P in M a bilinear function g_P on the tangent space T_PM. Let fP stand for $f(P)$, the image of P by the permutation f. Likewise, if V belongs to T_PM, the image of V by the induced isomorphism of T_PM onto $T_{fP}M$ will be denoted by fV. Then, f is said to leave g invariant if and only if, for any pair of tangent vectors V, W at any given point P of M, $g_P(V, W) = g_{fP}(fV, fW)$.

theory's models—, each consisting of an underlying world and a list of geometric objects defined on that world and satisfying certain postulated conditions. (Note, by the way, that it is therefore possible to 'generalise' a given theory by relaxing one or more of its postulated conditions, or to specify a subtheory of it—*i.e.* a subclass of models—by adding more conditions.) The i-th geometric object of a space-time theory T is *absolute* in Friedman's sense if and only if any two models of T, $\langle M, F_1, \ldots, F_n \rangle$ and $\langle M, G_1, \ldots, G_n \rangle$, which share the same underlying world M, meet the following requirement: for every point P of M there is a diffeomorphism (a smooth one-to-one mapping with smooth inverse) f of a neighbourhood A of P onto a neighbourhood B of P such that, on the intersection of A and B, $G_i = f_* F_i$ (where f_* is the mapping induced by f on the bundle to which F_i belongs). At first sight it would seem that this mathematical characterisation cannot capture the intended, essentially physical distinction between 'absolute' and 'dynamic' objects. But on a closer look one might be more willing to concede that objects satisfying Friedman's rigid demands simply cannot serve as suitable representatives of physical fields, inextricably linked to one another in the dynamic give and take of nature. However, the characterization is not unexceptionable. On the one hand, it may be too wide, for it implies that in the sub-theory of GR in which matter is pressureless dust (stress-energy tensor $T^{ab} = \rho U^a U^b$) the worldvelocity of matter (U^a) is an absolute object. Friedman, who mentions this counter-example on page 59, note 9, claims that it can be avoided by preferring for the said subtheory the 'more natural' formulation in which, instead of the worldvelocity U^a *and* the density ρ, it is their product, the world-current density ρU^a, which is designated as a distinctive geometric object. But this sort of reply will no doubt smack of *ad-hoc*-ness to many readers. On the other hand, Friedman's characterisation of absolute objects may be too narrow. For suppose that inspired by a view of spatial geometry favoured by the founders of Non-Euclidian geometry, we reformulate NK as a space-time theory in which the underlying world M is foliated as usual by absolute time, but the spatial geometry induced on each leaf of the foliation by a degenerate Riemannian metric g on M is a geometry of constant nonpositive curvature k—the same for all times. The theory has at least one model for each admissible value of k, and, following Lobachevsky, we imagine further that it is up to experience to tell us which is the model we live in. Since g, with the stated latitude, is postulated *a priori* as a 'background' geometry, not subject to dynamical influences, one would expect it to be classified as an absolute object of the theory, and yet it does not satisfy Friedman's criterion. I grant that the liberalised NK I have sketched is not a plausible space-time theory; but methodological notions, such as that of an absolute object, should be liable to be extended without pain to every conceivable application and not be restricted to those which are already familiar.

The difficulty in coming up with a satisfactory explication of Anderson's idea is not surprising, for the intended intuitive notion of absolute and

dynamical objects has not much to do with the symmetries of space-time. Thus, unless the stress-energy tensor vanishes everywhere in a class of models of *GR* one would not wish to describe it as an absolute object of the respective subtheory. And yet, most known families of solutions of the Einstein field equations admit symmetries which leave the stress-energy tensor invariant. (Such symmetries are often the key to the discovery of the solutions.) I should also mention that in ordinary mathematical parlance, as opposed to Anderson's peculiar idiom, the symmetry group of an *arbitrary* *GR* space-time is not the extra-large group of all smooth permutations, but the extra-narrow one consisting of the identity alone. Indeed, if one insists in using the expression 'symmetry group of *GR*' as Anderson and Friedman do, one should aver that there are infinitely many such groups, *viz*. one for each family of globally diffeomorphic models of the theory.[1] I am not sure, therefore, whether Friedman has made a good investment of his time in trying to salvage Anderson's idea about absolute objects and symmetry groups, which anyway he is not using to vindicate Einstein's name for his theory of gravity (*cf*. p. 214), and which I do not think is relevant to the important points that Friedman wants to make.[2]

The two final chapters of Friedman's book deal, under the headings 'Relationalism' (pp. 216—63) and 'Conventionalism' (pp. 264—339), with some genuine and alleged philosophical consequences of Relativity theory. Let us take up the last one first. Epistemological conventionalism results from subscribing to Kant's clear-cut distinction between the sensuous and the intellectual factor in knowledge while at the same time dismissing Kant's views about the uniqueness and necessary validity of the latter. For a

[1] *Added in proof*. The two last remarks are granted, at least implicitly, by Hiskes [1984], in her newly proposed explication of Anderson's ideas. Hiskes introduces—under tortuous stipulations which I cannot review here—the notion of a *local dynamical symmetry group* of a space-time theory. She states that 'the class of local dynamical symmetry transformations' of *GR* 'may be identified as the class of all diffeomorphisms defined on [any manifold] *M*' (underlying, I presume, a model of *GR*), and contends that the relation which the local dynamical symmetry group bears to 'general relativistic theories' is 'very similar from the perspective of the theory's structure' to the relation which the Poincaré group bears to *SR* kinematics and electrodynamics. She then briefly argues against 'the claim that the Poincaré group of a special relativistic theory has a greater physical significance as a *symmetry* group than the class of local diffeomorphisms in a general relativistic theory'. I thank Adolf Grünbaum for bringing Hiskes' paper to my attention and providing me with a preprint.

[2] Another technical point which admits improvement is Friedman's distinction between the first-order and the second-order structure of a manifold endowed with a connection form. Not only is his definition of these expressions unclear, at least to me, but some statements in which he uses them can serve as premises for a valid argument with a false conclusion. Friedman writes on p. 188: 'If *M* is a general relativistic manifold then it has the same first-order structure as Minkowski space-time but not (in general) the same second-order structure.' Twelve lines later, he adds that in a manifold structured by a proper or improper Riemannian metric, 'the [covariant] derivative operator is determined by the first-order structure.' Since both *SR* (Minkowski) and *GR* space-times are structured by a Riemannian metric, the two quoted statements imply that there can be no difference at all between the covariant derivative operator on either type of space-time, inasmuch as the said operator is determined by the first-order structure which both types are said to share.

conventionalist such as Henri Poincaré, the conceptual apparatus by means of which the raw facts (*faits bruts*) of sense are transmuted into scientific facts (*faits scientifiques*) is to be chosen on grounds of convenience. Relativity theory has often been said to favour conventionalism in so far as it dismantled the classical system of physical geometry and chronometry that Kant had declared a permanent feature of human reason. Friedman fights this claim respectfully yet firmly. Without getting at the root of the evil, *viz.* the alleged cleft between sense and understanding, he links conventionalism to the underdetermination of general statements by particular observations. This can be of two kinds: some general statements are underdetermined because our observations are too few—thus, for example, 'there are an infinite number of distinct smooth curves through a finite set of data points' (p. 266)— but others would remain underdetermined even in the light of every possible observation. If the meaning of a statement were its method of verification, then any two sets of statements descriptive of a given field of phenomena and equally well confirmed by every possible observation in that field should be regarded as having the same meaning, so that a preference for one of them could only be a question of taste. But if we reject, as indeed we must, the verificationist criterion of meaning, underdetermination of the second kind must be resolved by appealing to rational methodological principles. As we shall soon see, in the chapter on Relationalism Friedman makes an important proposal for vindicating one such principle. In the chapter we are now considering he remarks further that underdetermination of the first kind can fare no better than the other one, for there is no established system of inductive logic.

> There are not two different epistemological problems, one relating to our methodological criteria for making lower-level inductive inferences, the other relating to our criteria for choosing higher-level theories. Rather, there is only the single problem of *nondeductive* inference. We are given a finite amount of evidence, but an infinite number of incompatible hypotheses conform to this evidence. [...]. Consequently, we need additional methodological criteria, standards of simplicity, economy, and so forth, that go beyond mere conformity to evidence. These criteria can be justified only by showing that they tend to produce true hypotheses in actual scientific practice in the real world. (Hence, any justification of our actual inductive methods must itself be empirical [...] This involves us in an inevitable, but not necessarily vicious, circularity) (p. 273).

Friedman goes on to discuss the conventionalist teachings of Reichenbach and Grünbaum concerning physical geometry and the definition of simultaneity in *SR* for a given inertial frame. His treatment of the latter is particularly complete and satisfactory (pp. 165—76, 309—20). Friedman examines Reichenbach's doctrine of non-standard synchronism ($\varepsilon \neq 1/2$) with admirable forbearance, clarifying its confusions and pausing to explain such choice dainties of contemporary epistemological scholasticism as ε-Lorentz transformations. The upshot of it all, as I see it, may be briefly put thus (though Friedman would perhaps disapprove even of my minor concession to conventionalism). Under the principles of *SR*, to synchronise distant clocks at rest on an inertial frame in a manner compatible with the

law of inertia and the natural order of time along the worldline of any ordinary massive material particle is tantamount to foliating *SR* spacetime into parallel spacelike hyperplanes. Hence, there is a three-parameter family of distinct synchronisations meeting the said conditions. To each member t of this family there is exactly one inertial frame F in which t constitutes standard synchronism. F and t are uniquely associated on strictly geometric grounds inasmuch as the leaves of the latter are Minkowski-orthogonal to the worldlines of particles at rest in the former. To synchronise clocks by t in a different frame F' amounts therefore to dating events relative to F' by the standard ($\varepsilon = 1/2$) time of F. By doing so, one arbitrarily privileges a direction—*viz.* the direction in which F moves uniformly relative to F' (= the direction of minimum ε in the chosen non-standard synchronism for F')—in a space that was assumed isotropic (Euclidian) to start with. Of course, anyone can set his clocks by such a rule, if he so desires. He may have practical difficulties in keeping track of the priviledged direction, but his choice does not involve a more recondite freedom than, say, to set your watch by Greenwich Meridian Time when you live in Manitoba. Thus, the variety of ways in which events in an *SR* world can be partitioned into simultaneity classes adapted to inertial reference frames is fixed once and for all by the structure of *SR* space-time and is not enriched by Reichenbachian conventionalism. All the latter does, in effect, is to license the pairing of the inertial frames with the available—standard—synchronisms in every conceivable match (or mismatch).

Relationalism, as discussed in Friedman's book, is the doctrine, presumably initiated by Leibniz, according to which the physical reality of space and time (and space-time)—supposing they have any—must be fully accounted for by the reality of material substances (bodies?) and their mutual relations. Newton, following More and Barrow, postulated the existence of 'true, absolute or mathematical' space and time, as distinguished from the relative spaces and times that men can construct from observable bodies and physical processes. In a posthumously published manuscript, he pointed out that space and time could be neither substances nor attributes, as conceived by traditional ontology, but belonged in a category of their own. However, philosophers do not relish the multiplication of ontological categories beyond what they have learnt at school, and, having well-founded reservations against the assimilation of space and time to substances, they have tended for the most part to downgrade them to relations—or rather systems of relations—between substances. By thoroughly relativising space and time to moving bodies (frames), *SR* resolved the Newton-Leibniz dispute, as classically formulated, in favour of the latter; but only to raise again the dreaded spectre of a Newtonian absolute in the form of space-time. The space-time physicist who takes with Friedman—and with Newton and Einstein—a realist view of physics is committed to the existence of a model of his theory, *i.e.* of a four-dimensional manifold endowed with the additional structure determined on

it by the geometrical objects designated by the theory. A model of a space-time theory is often referred to as a set of *events* standing in certain definite relations to each other. This might seem to give encouragement to relationalists. But the physical events a space-time is built from are twice removed from what commonly goes by that name in everyday language. In the first place, they are unextended and durationless, *viz*. pointlike events at an instant. In the second place, they may be just 'possible' events, that is to say, locations open, in a relational system, for actual physical events. To account for space-time in terms of ordinary events the relationalist must therefore deconstruct the two-tier idealisation that took us (*a*) from events extended in space and time to point-events, and (*b*) from physically occupied such points to empty ones. Friedman sees no problem in step (*a*), as he apparently believes that physical events can be realistically analysed without more ado into pointlike instantaneous ingredients. But he gives a detailed and very instructive discussion of (*b*).

The difference between the absolutist's understanding of space-time and that of a relationalist who feels no qualms about step (*a*) turns essentially on their different interpretation of the relationship between the space-time manifold and the physically real, occupied point-events. For the absolutist, space-time as such enjoys *bona fide* physical existence, and the set of occupied point-events is to be regarded simply as a proper subset of it. For the relationalist, only the occupied point-events can claim reality; their complement in space-time, the so-called 'possible events', serves only to round up the space-time structure into something less unwieldy than the set of true events and their actual relations. Thus, for the absolutist, the relation between occupied point-events is one of *inclusion*; for the relationalist, it is one of *embeddability*. In Friedman's terminology, a set A, structured by relations $R_1, \ldots, R_m$, is embeddable into a set B, structured by relations $S_1, \ldots, S_n$, if and only if (i) $m \leqslant n$, (ii) there is a one-to-one mapping q of the first m positive integers into the first n such that, for $i \leqslant m$, S_{qi} is a p-ary relation whenever R_i is p-ary, and (iii) there is a one-to-one mapping f of A into B such that, for each $i \leqslant m$ and suitable p, $R_i(x_1, \ldots, x_p)$ if and only if $S_{qi}(fx_1, \ldots, fx_p)$. The mapping f is then said to be an *embedding* of $\langle A, R_1, \ldots, R_m\rangle$ into $\langle B, S_1, \ldots, S_n\rangle$.[1] A simple example of embedding, in this sense, is the assignment of space and time coordinates to physical events. The set of the latter, as structured by some sort of topological nearness, is thereby embedded into $\mathbf{R}^4$. Note that many aspects of the standard structure of $\mathbf{R}^4$—*e.g.* the status of (0, 0, 0, 0) as the neutral element

[1] Note that this use of 'embedding' is not synonymous with the standard meaning of the word in differential geometry, where it is employed to designate a smooth one-to-one mapping of a differentiable manifold into another, which meets certain specific topological and algebraic conditions. To be embeddable in a space-time in this standard geometric sense, the aggregate of real events would have to constitute a differentiable manifold (diffeomorphic to a submanifold of the space-time in question)—an assumption that can never be substantiated by the kind of empirical evidence a relationalist is likely to accept.

of the underlying additive group of a 4-dimensional real vector space—play no role whatsoever in the representation of events by this embedding. (It is also worth noting that most structural features of events are disregarded as well. In general, when one embeds experienced realities into a mathematical structure, the embedded structure never actually consists of the former as such, but is distilled from them by abstraction, *i.e.* by ignoring some of their features—and idealisation—*i.e.* by streamlining those that remain.)

Friedman concedes that empirical evidence concerning a collection of real events can only warrant its embedding, not its inclusion, in a model of a space-time theory. This implies that the relationalist, but not the absolutist, may draw support from such evidence. However, this does not settle the philosophical dispute between them in favour of the former. For, as Friedman notes, the inclusion of real events in a space-time structure—and hence the absolutist's understanding of the latter—can be vindicated on the strength of a rational methodological principle. It is at this point that Friedman makes his boldest and, to my mind, his most interesting philosophical move. He acknowledges—on page 242, note 14—that William Whewell anticipated it to some extent in his doctrine of the consilience of inductions; but, apart from an earlier paper by Friedman [1981], I do not find anything like it in recent epistemological literature. Let me try to summarise the gist of it. If I embed several physical situations $S_1, \ldots, S_n$ into a space-time structure S, I represent the features of interest of $S_1, \ldots, S_n$ by some, not necessarily the same, features of S. $S_1, \ldots, S_n$ are therefore given, by their respective embeddings $f_1, \ldots, f_n$, distinct, independent mathematical representations, which throw little or no light on one another. On the other hand, if the point-events discernible in $S_1, \ldots, S_n$ are literally identified with points of S, the mappings $g_1, \ldots, g_n$ that send the point-events of $S_1, \ldots, S_n$ to those very same points regarded as elements of S are suitable restrictions of the canonical inclusion of the union of $S_1, \ldots, S_n$ into S. Consequently, the representations of $S_1, \ldots, S_n$ provided by $g_1, \ldots, g_n$ must be consonant with each other, and we must also expect *every* structural feature of S to be relevant to the physical situations $S_1, \ldots, S_n$. In so far as this requirement and these expectations are fulfilled, the theoretical structure S unifies the experienced situations $S_1, \ldots, S_n$, thereby providing a way of confirming our understanding—and testing the reliability of our observations—of each of them by means of our knowledge of the others. Before illustrating this idea in detail through its application to space-time theories, Friedman gives an example drawn from a different field of physics:

> The hypotheses that collectively describe the molecular model of a gas of course receive confirmation via their explanation of the behavior of gases, but they also receive confirmation from all the other areas in which they are applied: from chemical phenomena, thermal and electric phenomena, and so on. By contrast, the purely phenomenological description of a gas—the nonliteral embeddability claim—receives confirmation from one area only: from the behavior of gases themselves.

Hence, the theoretical description, in virtue of its far greater unifying power, is actually capable of acquiring more confirmation than is the phenomenological description.

This may seem paradoxical. Since the phenomenological description of a gas is a logical consequence of the theoretical description, the degree of confirmation of the former must be at least as great as the degree of confirmation of the latter. My claim, however, is not that the phenomenological description is less well-confirmed than the theoretical description after the former is derived from the latter—this is of course impossible. Rather, the phenomenological description is less well-confirmed than it would be if it were *not* derived from the theoretical description but instead taken as primitive. The phenomenological description is better confirmed in the context of a total theory that includes the theoretical description than it is in the context of a total theory that excludes that description. This is because the theoretical description receives confirmation from indirect evidence—from chemical phenomena, thermal and electrical phenomena, and the like—which it then "transfers" to the phenomenological description. If the phenomenological description is removed from the context of higher-level theory, on the other hand, it receives confirmation only from direct evidence—from the behavior of gases themselves.

In other words, a total theory rich in higher-level structure is likely to be better confirmed than a total theory staying on the phenomenological level, even though the latter theory may have precisely the same observational consequences as the former (pp. 243 ff.).

There is not much I could add by way of explanation to Friedman's own lucid words on this matter, but I still have three remarks to make concerning it. First of all, it seems to me that the identification of observable situations and processes with parts of a comprehensive major structure does not only serve to coordinate the scientific descriptions of those situations and processes and to ensure their mutual support, but works also as a guiding principle that makes it possible to achieve such description in the first place. Thus, to give a very simple example, it is practically inconceivable that Kepler could have ever thought of constructing an elliptical orbit from Tycho Brahe's data on Mars if he had not assumed as a matter of course that the planet's positions were points in an Euclidian space. Secondly, we must bear in mind that the identity of the elements of a space-time structure, and hence their 'literal identification' with real point-events, can only be determined up to an automorphism of the structure (*i.e.* up to a permutation of the underlying set that leaves all structural features invariant). Newton's denial that space and time are substances is based on the fact that they both share this feature. As he notes in the posthumous manuscript I mentioned, '*it is only by their mutual order and positions that the parts of time and space are understood to be the very same which in truth they are, and they do not possess any principle of individuation apart from that order and those positions.*'[1]

[1] Hall and Hall [1978], p. 100. The crucial importance of this ontological peculiarity of space-time points for Einstein's final rejection in November 1915 of his 1913/14 'hole argument' against the possibility of generally covariant gravitational field equations was pointed out to me by John Stachel in the Fall of 1980. At that time I resisted Stachel's interpretation for reasons spelled out in Torretti [1983], pp. 167 ff, but in the mean time I have been slowly won over to it. *Cf.* Stachel [1980].

Finally, I must say that I find it utterly naïve to think that the verb 'to be' as applied to a space-time structure, whose elements possess identity only up to an automorphism and can never be met in person but only through their blurred, evanescent corporeal manifestations, can mean the same as the verb 'to be' as applied to the tree in the quad or to the three ice cubes now melting in my drink. It follows from this that, if you share—as almost everybody does nowadays—the dreary belief that there is no better reality besides the reality you have to hand, you must look down upon the reality of space-time, which only gives food for thought, as being one of a lesser or weaker kind. But you ought not to boorishly rush to the conclusion that unless space-time can be formally reduced to such things as tables and beer mugs, it is no more than a fiction. *Tò ón légetai pollakhôs*—being is meant in various ways—, as Aristotle said; and within that variety there is surely enough room for the objects of sense which we enjoy and for the intelligible structures by which we seek to understand them.

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